The practice of quoting the titles of foreign papers is aggravating in the extreme, especially when accompanied by the remark that "this laborious but lengthy paper is not suited for 'abstraction!'"

I seldom go out of our foggy little island without hearing our want of familiarity with what is going on elsewhere roundly and deservedly abused. But I feel bound to say that, more especially in France, I can justly retaliate.

Russian science is said to be very productive. That language, at all events, is beyond most of us.

New University Club MARSHALL HALL

Aurora Australis

OBSERVING in NATURE of Oct. 27, an account of a brilliant aurora observed in England on the 23rd to the 25th of that month, I think it may excite attention to the subject by informing your readers that at the same date a splendid display of Aurora Australis was recorded at most of the meteorological stations in this colony.

The extent of sky covered was not so great as in the fine display on 5th April last, which was also coincident with a remarkable Aurora Borealis, but the red tint was so much deeper than usual, that many persons in this place attributed the phenomenon to the glare of the great fire which destroyed the town of Lyttelton, and the news of which was just then received by telegraph.

JAMES HECTOR

Colonial Museum and Observatory, Wellington, N.Z., Jan. 2

Ocean Currents

MY rejection of the idea that permanent differences of atmospheric pressure could produce any effect on Ocean Currents, was meant to be as sweeping as Mr. Johnston considers it. I believe that the idea is repugnant to the most elementary conceptions of hydrostatic equilibrium; and I am particular in so far repeating the gist of my former letters, because Mr. Johnston, in his letter in NATURE for March 9, reiterates his suggestion that difference of atmospheric pressure is a power in the production of Ocean Streams, and whether he suggests that it is a supplementary power, or a chief one, is nothing to the purpose, if, as I distinctly maintain, it is not a power at all.

My rejection of the idea that the formation of these differences of pressure can produce any appreciable effect, is quite as decided; but Mr. Johnston is mistaken when he speaks of my denying also the influence of the movement of these differences of pressure; for my remarks concerning them were to the very opposite purport; and I pointed out that such movements do sometimes give rise to rapid and dangerous sets, known as storm-currents, which in their irregular and exceptional nature, differ essentially from those regular permanent or periodic currents usually understood by the general term Ocean Currents, though they may occasionally modify them both in direction and velocity.

I would also call Mr. Johnston's attention to the fourth paragraph of his letter, and assure him that I have never, directly or indirectly, maintained that the Trade Winds "would account for the whole of the phenomena of Ocean Currents;" but I have maintained, and do still maintain, that all the phenomena alluded to may be very satisfactorily accounted for by a reference to the prevailing winds of the different parts of the world; and that the Gibraltar Current is to be attributed, not to the local, partial, and peculiar wind of the Straits, but to the great body of the west wind of the North Atlantic, which also produces a northerly current on the coast of France, known distinctively as Rennell's Current, and a southerly current on the coast of Portugal.

I have discussed this question so fully in another place, that I should be only repeating myself were I to say more about it here; but I may add that though, as Mr. Johnston asserts, under-currents cannot be caused primarily by the action of the winds, they can be, and frequently are, caused secondarily by that action; and many a ship has owed her safety from the apparently imminent danger of a lee-shore, to the "under-tow," or reflux of the water swept towards the shore on the surface. If there is a deep-flowing outward current in the Straits of Gibraltar, I believe it to be exactly of the nature of an "under-tow;" it seems to me probable enough that there is occasionally such an outward current; but I cannot admit that the *one* observation of it which Dr. Carpenter considers he obtained, after several attempts made in vain, has abundantly proved its existence; still less can I admit that it is necessary to call in difference of temperature and density to account for it.

Mr. Croll considers that there is a similar escape of water, underneath, from the northern to the southern hemisphere, and his arguments appear to warrant the suggestion, although no such under-current, or system of under-currents, has yet been observed. I see no improbability in the idea; but so many mistakes have, at different times, been made by trusting rather to theory than to positive evidence of fact, that we cannot be too cautious in admitting the existence of such under-currents, without any reliable observations. For that recorded by Captain Maury, to which Mr. Johnston refers, has, from the vague manner in which it is described, no scientific value whatever. There is no mention of locality, season, direction of wind, swell or surface current, no mention of the relation between the effective area of the "block of wood loaded to sinking" and the barrega or breaker which floated it; the depth is spoken of as indifferently one hundred or five hundred fathoms; above all, no mention is made of any means being taken to distinguish between an apparent and real set of the breaker. It is quite clear that if the loaded block was lowered into still water, the breaker to which it was attached was, to a certain extent, moored, and the surface drift of the boat away from it would give it all the appearance of moving in the opposite direction. No mention is made of the method adopted to discriminate; or, in fact, of any method at all being adopted, or any attempt made to eliminate or neutralise the many errors which necessarily find their way into such an observation; all that we are told is that "it really appeared as if some monster of the deep had laid hold of the weight below, and was walking off with it." To such an account one is almost attempted to add—very like a whale.

I have dwelt on the thoroughly unsatisfactory nature of this experiment, because, from the description of it having been repeated in every edition of the "Physical Geography of the Sea," I find it constantly referred to—as Mr. Johnston has now referred to it—as a conclusive proof of the existence of strong counter under-currents at great depths; whereas in reality it is a proof of nothing, unless, perhaps, of the careless style of observing which was accepted as sufficient twenty years ago.

The other instance which Mr. Johnston brings forward would be really remarkable, if we only had some evidence of it as a fact; he speaks of the warm water of the Atlantic dipping down beneath the cold and "specifically lighter" water of the east Greenland current. It has been well known, long before the late German expedition, that at the meeting of the two waters there is a distinct line of demarcation, but such a line does not necessarily indicate a dip of either water, such as Mr. Johnston describes; as indeed has been very fully shown by the survey of the nearly vertical "cold wall" of the United States, along which the line of demarcation is more distinct than anywhere in the world. And besides, can we admit that the water of the East Greenland current is "specifically lighter" than that from the Atlantic? that the cold water is lighter than the hot, the salinity of the two being very nearly equal? Captain Maury speaks of hot water, like oil, running over cold; Dr. Carpenter illustrates the same idea in a long glass trough, showing plainly enough the way in which he conceives the interchange to take place. I do not attach so much value as Dr. Carpenter does to this illustration, which represents a system of motion entirely different from that of the ocean-currents; but accepting it as the exposition of the views held by the leading supporters of the claims of temperature and density, it is utterly antagonistic to the idea of this extraordinary dip of warm water said to take place near the east coast of Greenland. Whether we consider it from a purely theoretical or from a geographical point of view, the idea is wholly unsupported, and can only be classed as one of those crude speculations which, in every branch of science, do so much harm by tending to unsettle the minds of those who indeed take an interest in the subject, but have not made it a special study.

J. K. LAUGHTON

Draper's Experiment simplified

WISHING to repeat Draper's Experiment, and casting about for a simple method of performing it, it occurred to me to take advantage of the intense heat evolved in the combustion of sodium, and, beginning with the entire spectrum, watch its degradation as the heat declined; to which end I formed a shallow, conical cup of thin copper wire, half an inch in diameter, and, putting therein a piece of sodium, applied a spirit-lamp till

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it burst into flame; very soon the mass melted and rose to an intense, white heat, the air streaming in through the spiral greatly favouring the combustion, a full spectrum of the utmost purity and splendour was formed, which continued as long as the white heat lasted, but afterwards declined from, and rapidly at, the violet end through the whole spectrum to the red, which persisted longest. On repeating the experiments, and raising a very narrow slit to the spectroscope, I found, as I anticipated, the sodium line reversed, and I had before me a miniature sun, a glowing centre emitting light of every wave-length, while the melted sodium, flowing through the intervals between the wires of the lower part of the cone and being dissipated by the heat, surrounded the liquid centre with an atmosphere absorbing light of its own refrangibility.

Thinking some of your readers would like to repeat so simple and inexpensive an experiment, I have ventured to send you this. T. F.

St. Mary Church, Torquay

A Wind-direction Rain-gauge

In your issue for yesterday, March 30, at page 433, you give a paragraph respecting a "Wind-direction Rain-gauge." Might I be allowed to observe that a gauge on the same principle has been in use at this place for many years, "it is arranged for *four vessels*" only, showing N. to E., E. to S., S. to W., and W. to N. Wm. Lyall

Literary and Philosophical Society, Newcastle-upon-Tyne

Entomological Queries

Can any of your entomological readers refer me to any works or memoirs on British Ants published since the appearance of Westwood's "Classification of Insects," and not quoted by Mr. Smith in his volume on "Formicidæ" in the British Museum Catalogue? What is the scientific name of the Texas agricultural ant and of the smaller ant which it ejects from its colonies? And where can I find M. Lespes' paper or papers on the "Domestic Economy of Formicaries," especially with regard to the Clavigers and other blind beetles? A. Emmet

Feb. 21

Rain produced by Fires

In your No. of Feb. 16, there is a letter from Mr. Laughton on the Artificial Production of Rains, which is worthy of notice from a strictly scientific point of view. I have little doubt that rains have been in comparatively rare cases caused by large fires. We may dismiss from our minds the idea that rains can be produced, even when the conditions are favourable, by all the powder that is burnt during a battle on land or sea. It is said that "in a problem of this nature, negative examples have more weight than positive." But it is surely more philosophical to hold that the one class of instances is as valuable as the other. If rains have sometimes been produced by fires, it is as well to try to eliminate the conditions under which they occur as in those cases in which they do not occur.

It is curious enough that much of the popular belief as well as a disbelief in the connection between rains and fires must be ascribed to the late Prof. Espy. After laying down with scientific accuracy the atmospheric conditions for such an occurrence, he somewhat lost sight of the principles in his instructions to the farmers of the United States for burning their felled timber for the production of rain. I need not enter into these. The negative cases are found to be numerous enough. Great fires rage over the Prairies and through the woods in America for weeks during the autumn, and the air becomes darkened by a veil of smoke, while no clouds are to be seen. This usually occurs when the wind is from the west and the air dry, and naturally blue and bright till obscured by smoke. In such instances the theory of fires producing rains justly enough becomes unpopular.

On the other hand Espy laid down with great precision the conditions under which rains will result from great fires in "a high dew-point and a calm atmosphere." In short, the air must be pretty well saturated with moisture, and verging upon that unstable state of equilibrium under which cumulus clouds are formed. As Espy has shown, and every observer of the phenomena will confirm, the gorgeous cumulus clouds of summer are not seen when the air is much disturbed by winds. Their very formation and existence depend upon ascending currents. Previous to Espy's investigations, it was supposed that the formation of cumulus was due to the expansion of the vapour of water by the heat of the sun and its consequent diffusion upwards through the permanent gases till it was condensed above. He clearly showed by experiment that vapour has little power of permeating air under the ordinary pressure of the atmosphere. And he drew the inference that it could be only carried into the higher strata by ascending currents. This, I think, is amply borne out in the formation of all clouds.

Fires then, are only likely to produce rains during comparatively calm weather. In the positive instances given by Espy, most of the observers state that the air was calm and sultry. One of the staff of the United States surveyors when in Florida mentions, that by firing the bush "whenever there was no wind stirring, we were sure to get a shower." Great fires are comparatively rare in this country, and I have never seen the formation even of cloud here from such a cause. However, I did once observe this phenomenon. Many years ago when sailing up the Mississippi near its mouth, in a clear and cool evening, after the subsiding of a "Norther," a great fire was burning among the reeds on its west bank. Above the dark smoke the true cumulus cloud was distinctly formed. Its bright and rounded form was beautifully brought out in the setting sun. No other clouds were visible around, and these were soon left behind as we ascended the river.

If we reflect on the matter it is difficult to conceive how clouds could be formed by means of fires during windy weather. The ascending columns could not be formed under such conditions, for the heated air would be rapidly swept off, and diluted with the mass of air rushing past. This may be illustrated by other examples better known than the formation of the cumulus. Volcanoes are well known to produce at times clouds as well as rains. But all the vapour or heat that Vesuvius could emit during an active eruption would not produce rains when a strong and dry north-west wind was blowing across its top. So also the beautiful and true cumulus cloud that so often hovers over the Falls of Niagara is only seen in calm weather. Under favourable atmospheric conditions I have lately been informed by Dr. Henry Washington that the Niagara cloud sometimes gives rise to rains and electrical phenomena. The true inference seems to be that great fires will not produce rain, excepting "the air is calm, and the dew-point high." Robert Russell

Pilmuir, Leven, Fifeshire,

A BILL TO ESTABLISH THE METRIC SYSTEM OF WEIGHTS AND MEASURES

The following Bill, prepared and brought in by Mr. J. B. Smith, Sir Charles Adderley, Sir Thomas Bazley, Mr. Graves, Mr. Baines, Mr. Albert Pell, Mr. Muntz, and Mr. Dalglish, has been ordered to be printed by the House of Commons:—

Whereas it is desirable that the weights and measures of the United Kingdom should be decimalised, and made to correspond with those of other countries.

And whereas the use of metric weights and measures is now legal, but no provision has been made for procuring the standards of said metric weights and measures, and for verifying and stamping those in use under the said Act.

Be it enacted by the Queen's most excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons in this present Parliament assembled, and by the authority of the same as follows:—

1. From and after the expiration of ( ) years after the passing of this Act, the length of the metre to be prepared under the authority of the Privy Council for Trade, verified by comparison with the original standard in Paris, having the words and figures "Standard Metre, 1871," engraved upon it, and kept in the custody of the Warden of the Standards, shall be and is hereby declared to be the unit or only standard measure of lineal extension, wherefrom or whereby all other measures of extension whatsoever, whether the same be lineal, superficial or of capacity, shall be derived, computed, and ascertained, and all such measures shall be taken in decimal multiples or decimal parts of their respective units.

2. The unit of the measure of surface shall be the square of ten metres, which shall be and is hereby denominated the "are."

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